generated in the sky by a fulgureous exhalation conglobed in a cloud by the circumfixed humour, and are as it were baked hard by intense heat, and the weapon becomes pointed by the damp mixed with it flying from the dry part, and leaving the other end denser, but the exhalations press it so hard that it breaks out through the cloud, and makes thunder and lightning. But, he says, if this be really the way in which they are generated, it is odd that they are not round, and that they have holes through them, and those holes not equal through, but widest at the ends. It is hardly to be believed he thinks."[1]

Here we have an example of a brilliant and comprehensive theory—a theory able to explain everything, yet subject to petty criticism! And we fear that our anonymous author's equally brilliant theory of the orgin of the whale will be not less unfortunate. Of course we are assured that the theory explains almost everything—homology, embryology, rudimentary organs, &c., though he does, modestly, admit that it does *not* explain why hybrids are sterile. In order not to misrepresent the writer one more passage must be quoted, because he there brings his ideas more nearly into accord with that theory of discontinuous variation which has been recently put forward.

> "Evolution proceeded by successive distinct gradations or stages. The differentiation of every new species resulted from forces *ab extra* superimposed on, and, to some extent, superseding or modifying the forces that produced the species or genus immediately preceding in the same line of development. The fecundated ovum of a species was, as it were, fecundated a second time with a new force, and the ovum thus bi-fecundated produced, instead of the species to which it belonged, a new species built upon a modification of its predecessor."

The theory is therefore one of special creation through the ordinary process of descent. The "new forces *ab extra*" which produced a whale from a terrestrial animal were also at work every time one species of tit, or warbler, or beetle, or snail, was modified in adaptation to a slightly different mode of life, and became a new species. Thus all is explained; except why there is any variation of these specially adapted species, why they increase at such an enormous rate necessitating such wholesale destruction, why there is any struggle for existence. All these phenomena, which are the very essence of a theory of descent with modification by natural selection, are entirely out of place in a theory of special creation, and are therefore the condemnation of any such theories.

ALFRED R. WALLACE.

THE MEAN DENSITY OF THE EARTH.

The Mean Density of the Earth. An Essay to which the Adams Prize was adjudged in 1893 *in the University of Cambridge.* By J. H. Poynting, Sc.D., F.R.S. (London: C. Griffin and Co., Limited, 1894.)

THIS essay, which contains an account of Prof. Poynting's well-known investigation of the mean density of the earth, though the last Adams prize essay, is the first to which that prize has been awarded for experimental work. We hope that it is the first of a long series of essays in which the candidates will attack the questions proposed by experiment as well as by mathematical analysis. We can hardly expect, however, that the level reached by the magnificent experimental work of Prof. Poynting will always be maintained.

The essay consists of two parts, the first containing an account of previous determinations of the mean density, the second an account of Prof. Poynting's own determination by means of the ordinary balance.

The first part begins with an account of the astronomical or geodetical methods, in which the attraction of a mountain was compared with that of the earth, as in the experiments of Bouguer in Peru, of Maskelyne and Hutton on Schehallien, of James and Clark on Andrews Seat, of Carlini on Mount Cenis, and of Mendenhall on Fujiyama; or with that of the slab of matter above the surface of a mine as in Airy's Harton Pit experiments, and von Sterneck's experiments in Pribram and Freiberg The beautiful method employed by von Sterneck in his pendulum experiments ought to be more widely known in England. The object of the astronomical method has undergone a curious reversal. It was originally to deduce the mass of the earth from a supposed knowledge of the distribution of matter in the locality of the experiment, whereas now it is rather to find the distribution of matter in this locality, assuming the mass of the earth to be known.

The other methods are laboratory methods, and depend upon the measurement of the attraction between known masses. Prof. Poynting points out a very interesting under-estimate of this attraction made by Newton. In the Principia, Newton estimated that two spheres of the density of the earth, each a foot in diameter, would, if separated by quarter of an inch and left to their own attractions, take nearly a month to come into contact. Prof. Poynting shows that there is a mistake in the arithmetic, and that in reality the spheres would come into contact in between five and six minutes.

It is now very nearly a century since the first measurements of the attraction between two masses in a laboratory were published by Cavendish ("Experiments to Determine the Density of the Earth," *Phil. Trans.* 1798), who used the torsion balance. Since then this method has been used by Reich, Baily, Cornu and Baille, and Boys; while the ordinary balance has been used by von Jolly, Prof. Poynting himself, and by König, Richarz and Krigar Menzel, working in collaboration, while the method of the pendulum balance has been used by Wilsing. The labour expended over these investigations may be estimated from the fact that, to take only two modern instances, Prof. Poynting's experiments extended over twelve years, while those of Cornu and Baille were commenced in 1870, and are not yet completed. The essay contains a clear and critical account of the preceding experiments. The result of the criticism is to raise, if possible, Cavendish's fame as an experimenter. Of Baily's laborious research, Prof. Poynting says: "The critical examination it has received in later years has entirely destroyed any confidence in the result. It remains, however, as a most remarkable and useful example of the danger of substituting multiplication of observations for consistency." The contrast between the amount of work which has been published on the numerical magnitude of the attraction, with that which

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[1] "Early History of Mankind," second edition, p. 227.

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has appeared on the effects, if any, which modifications of the surroundings exert on this attraction is very remarkable. Newton's hollow pendulum experiments, repeated with greater accuracy by Bessel, seem to be almost the only investigations which have been published on what may be called the physical properties of gravitational attraction. As far as we know, the attraction between two given masses depends merely upon their geometrical configuration; it is independent of the medium between them, of the physical state of the masses, whether they are solid, liquid or gaseous, amorphous or crystalline; it does not depend upon the temperature of the masses. Indirect evidence, often derived from the Cavendish experiments, shows that the preceding statements must at least be very approximately true. Again, chemical analysis is founded on the hypothesis that the weight of an atom of a chemical element is unaltered whatever chemical combinations it may form, or whatever the temperature to which it may be raised. It is, however, often difficult to tell the degree of approximation to the truth of the preceding statements which is indicated by such indirect evidence, and a direct experimental investigation to determine an inferior limit to the accuracy of some of the preceding statements would not be superfluous. Take the case, for example, of the statement that the weight of an atom cannot be altered by chemical combination: it would be for the advantage of science if this were proved with the utmost possible accuracy attainable by present methods for some definite chemical combinations. The question is of interest in connection with the view that the atoms of elements are aggregations of atoms of some primordial substance not very much lighter than hydrogen. The values of the atomic weights of the elements is inconsistent with this view if each atom of the primordial element retains its weight unaltered in the complex atom. If, however, it suffers a slight change of weight, then we might expect to find traces, though perhaps faint ones, of such a change in ordinary chemical combinations.

Another question which has excited some interest is a possible connection between the magnitude of gravitational attraction and temperature. Prof. Hicks has pointed out that Baily's results gave a value for the mean density of the earth which uniformly diminished as the temperature increased, indicating, if the effect is a real one, that the attraction between two masses increases with the temperature, and von Sterneck, in his experiments at Freiberg, found a remarkable relation between the temperature and the value of gravity. Prof. Poynting discusses these and other results, and comes to the conclusion that they are to be explained by other causes, and do not afford any evidence at all that the attraction between bodies varies with the temperature. The point is one which has a direct bearing on Prof. Poynting's own experiments, as the final value for the mean density is got by taking the mean of two sets of experiments, and as the temperature in the two sets differed by about 3·5° C., an uncorrected temperature effect would affect the result.

We are very glad to find from this essay that Prof. Poynting is engaged on an investigation as to whether the attraction between two crystals depends on the relative position of their axes.

The second part of the essay consists of Prof. Poynting's paper "On a Determination of the Mean Density of the Earth," published in the *Phil. Trans.* for 1891. This, in addition to the actual determination of the mean density, is almost a treatise on the method of using a balance so as to get great sensitiveness. The work is a model of patient care and skill, as well as of clearness of exposition, and we feel as we read it that the utmost has been made out of the apparatus and the method. Prof. Poynting is of opinion that it is only air-currents which prevent the balance being used with an accuracy far beyond anything hitherto approached; the ordinary balance is more sensitive than the torsion balance to air-currents, since these produce the greatest disturbance in the vertical direction, which is the direction of displacement in the ordinary balance. Prof. Boys has shown that to minimise the effect of air-currents the size of the apparatus ought to be reduced as much as possible. Prof. Poynting says that if he were designing his apparatus again, instead of using, as he did, an exceptionally large balance, he would go to the opposite extreme and use a very small one.

Of all the methods hitherto used to determine the mean density of the earth, the arrangement used by Prof. Boys seems to be the one capable of the greatest accuracy. There are, however, certain points about the method of the common balance, such as the simplicity of the most important measurements, and the absence of the necessity to determine a time of swing with great accuracy, which make it worthy of such a work as Prof. Poynting has devoted to it.

The mean density of the earth found by Prof. Poynting is 5·49. Prof. Boys' result is 5·53.

MINING.

A Text-book of Ore and Stone Mining. By C. Le Neve Foster, B.A., D.Sc., F.R.S. 8vo. Pp. 744, with Frontispiece and 716 Illustrations. (London: C. Griffin and Co., Limited, 1894.)

IN view of the paramount importance of the production of minerals to Great Britain, and of the constant enterprises for working gold ores and other minerals in most of our colonies, it is certainly remarkable that there has hitherto been no satisfactory systematic treatise on metalliferous mining available. It is a matter of congratulation, therefore, that so eminent an authority as Dr. Le Neve Foster has found time, with his many duties as H.M. Inspector of Metalliferous Mines, and as Professor of Mining at the Royal College of Science and Royal School of Mines, to fill up so important a gap in technical literature. His compendious volume will undoubtedly be warmly welcomed as an invaluable work of reference, not only by mining students, but by all English-speaking mining engineers. The subject is so extensive, that the author's task of keeping his text-book within moderate limits, without erring on the side of omission, was one of considerable difficulty. He has, however, been thoroughly successful; and the extremely methodical arrangement of the material obviates, as i

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